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XXIII.

ON PEIRCE'S CRITERION.

BY BENJAMIN PEIRCE.

MY DEAR SIR,—I perceive that the theory of my criterion has been frequently misunderstood. I presume this to be due in a great degree to the conciseness of the argument with which it was published; and I propose to remedy this defect.

The problem which I undertook to solve was the following. There being given certain observations, of which the greater portion is to be regarded as normal and subject to the ordinary law of error adopted in the method of least squares, while a smaller unknown portion is abnormal and subject to some obscure source of error, to ascertain the most probable hypothesis as to the partition of the observations into normal and abnormal. The principle adopted in my solution of the problem is the universally recognized doctrine that the measure of the probability of an hypothesis compared with other hypotheses equally probable in other respects is the probability that the event will occur under the hypothesis, and that the most probable hypothesis is that under which the event is most probable. This is the literal expression of the mathematical analysis published in Gould's *Astronomical Journal* for 1852. The Criterion has been used otherwise than in the Coast Survey, and especially by my friend Dr. Gould himself. Dr. Gould's tables have greatly facilitated its use, and his sound judgment given in favor of its validity is at least as valuable as that of any living geometer. It has also been much used by that excellent authority Mr. Schott, as in a letter hereto appended.

The evidence, by which certain observations are placed in the doubtful list and subjected to scrutiny, whether they should be rejected, must be exclusively the magnitude of the errors which they involve, when these errors are computed as if they were normal observations. This would not seem to be obnoxious to the charge of inconsistency, any more than is the ordinary *Reductio ad Absurdum*, in using a method as correct in an observation where it was finally rejected. An

hypothesis which leads to improbable results is itself improbable to just that extent.

It is not likely that a series of observations of any great extent was ever made, in which some were not rejected on account of the magnitude of the deviations from the arithmetic mean. The object of the Criterion is to systematize this species of rejection, and reduce it to a form of exact computation. Wherever it has been applied, I believe that it has been found to accord with the best judgment of observers and computers. This fact has been to me an agreeable surprise, which has more than balanced the unfavorable criticism, having its foundation in misconception. It has been a surprise, because the theory was altogether *a priori*, and independent of comparison with observation.

I might add concerning the charge of inconsistency, that it would seem to be almost equally applicable to the case where two sets of observations made by different observers, and the arithmetical mean of each set differing materially from the arithmetical mean of the whole, the difference is explained by personal equation. The argument for the hypothesis of personal equation must rigidly assume the same form with that by which the Criterion is established.

There might be a doubt as to the reality of such abnormal observations with their obscure sources of error. I am frank to admit that in most cases science will detect the system of the sources of error, and free the observations from them. But even such a case as the familiar one of writing down a wrong figure must be included among these sources, and is evidently insusceptible of correction; whereas, if it be sufficiently large, it will be eliminated by the Criterion. Another case, which must often have occurred in transit observations by eye and ear, depends upon the erroneous mental sub-division of the record, of which I have given the analysis in another place. I can point out many observations which were manifestly erroneous on this account, and which it would be too vast a labor to undertake to rectify. Before the judgment of the Criterion all such errors disappear, if they are large enough to be of serious injury.

That some of the observations which are not rejected may be abnormal, notwithstanding the smallness of their errors, must be admitted. This possibility was fully recognized in the geometrical development which was given in the *Astronomical Journal*; and I am not aware that there has been any criticism adverse to the mathematics of that article.

TO HON. C. P. PATTERSON,
Superintendent U. S. Coast Survey.

NOVEMBER 22, 1877.

Respecting Prof. Peirce's Criterion, I venture a few remarks : —

Large errors may arise either from an *accumulation* of a number of small ones, having their origin in different sources, as is recognized in the theory of combination of errors, and as such they may be regarded as *normal* (even if quite large), provided they belong to a series of an indefinitely large number of observations. Practically, we have but a few observations (most frequently less than one hundred), and while certain sources of error may combine to the production of large ones, their *actual* appearance in a *short* series of observations must injuriously affect the most probable result (say the mean) deducible from the series. Here we need the means of separation, and the Criterion logically performs this.

Or large errors may arise from bad observations (due to inattention of observer, without being aware of it), from the presence in this particular case of an unsuspected constant error, or even from an accidental slip (necessarily not altogether outside the possibility of its being due to other causes admissible) : all such large errors having no recognized place in the adopted law of the occurrence of error must be subject to rejection, for which we need the Criterion.

Supposing the probable error of a result before rejection = ϵ and after rejection = ϵ_1 , the latter value has some times been adopted, which I think is generally not admissible, the value ϵ would better be retained as evidence that rejections have been made. If ϵ_1 is retained, we may fall into the error, of again trying on the Criterion* based upon ϵ_1 .

Having used the Criterion for the last twenty years in various investigations, I found it uniformly gave excellent discrimination, and do not remember a single case where it came in conflict with proper judgment based upon experience. Of course in those instances where we have antecedently a knowledge of ϵ , we would employ this value in preference to a value deduced from a *single* series of observations : in such cases, observations are frequently retained by the Criterion which otherwise would have been rejected. If it errs at all, it may sometimes appear to cut too deep ; but our general experience is that

* I remember a rather curious case of this kind, where an observer rejected successively three times, each time deducing and starting from a new probable error, when he became alarmed for the safety of the rest of his observations and stopped further testing them.

probable errors deduced by the method of least squares generally prove too small, whenever we can bring them to the test of different methods : this, of course, is due to the presence of unknown constant errors peculiar to each method.

Yours very respectfully,

CHAS. A. SCHOTT.

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